been watched here without indication of an associated star-shower. Still, the circumstance that certain meteoric displays have occurred contemporaneously with other phenomena, is interesting (though not, perhaps, significant, as intimating any physical connection), and such records should always be preserved, as possibly having a value which further observations may elucidate.

During the great meteoric shower of November 13, 1866, several observers detected a faint diffused light of an auroral character in the northern sky.

In 1880 there were bright displays of auroræ on August 11, 12, and 13. Whilst watching the Perseid meteor-shower on those nights, I noticed the successive appearances of streamers and light condensations in the northern quadrant. On the 13th the sky was much lighter than usual, though no streamers or bright glows were visible. The moon set before 10h., yet at midnight the air was not dark; objects around were discernible with remarkable facility beneath the luminous gauze of the aurora apparently diffused over the sky.

During the present month I have been engaged in a series of observations of the Geminid meteor-shower. On December 7, 8, 9, and 10, I especially noticed singular light-radiations, like broad films of faint white cloud, in the northern region. These were very striking on December 9, and persistent during several hours. The most conspicuous of these radiations stretched out of the horizon in the north-north-east, and ran obliquely a considerable distance east, where it enveloped the stars of Virgo and Leo. The effect was somewhat similar to that of the zodiacal light, but in the present case the phenomenon had an evident tendency to remain in the vicinity of the horizon. On its upper side I distinguished very faint indications of streamers, with alternating spaces rendered very dark by contrast. The appearances were, however, so constant, that they can hardly be associated with the characteristic mobile forms of ordinary auroræ. The sky generally was very light, and I have specially noticed this fact on several other occasions this month, when an observer might readily have assumed that the moon, in one of her quarters, was present in the firmament.

A suffused glowing of the whole sky such as that now recorded has not infrequently been visible here in past years during the progress of meteoric observations. Though no definite auroræ (in its normal features) can be descried, there is obviously some modification often present imparting to the firmament those peculiarities of aspect and tint which are far too striking to escape notice. I believe that scarcely a very clear night passes but there may be traced, with a critical eye, some feeble traces of auroræ, or their closely-allied phenomena. Could these ever-varying sky-tints be studied in a fine climate, I feel assured we might look for some interesting results.

Bristol, December 12 W. F. DENNING

The Supposed Fall of an Aërolite in Naples

THE late beautiful meteoric display, which was well seen here, has given rise to a somewhat ludicrous incident. The local papers on Sunday evening and Monday contained an account of an aërolite that had fallen in the Strada Fiorentino, one of the principal thoroughfares of the town; that this stone weighed between 6 and 7 kilogrammes, and had nearly struck some people who were passing. This announcement was followed by a description of the stone from the pen of one of the professors in the University, together with an account of meteorites in general.

Such a display of erudition, coinciding with the bombardment our earth has had from Biela's comet, prevented my sleeping all night, and, as early as etiquette would allow, I paid a visit to the house of the two professors, the *happy* (?) possessors of this would-be aërolite. The first examination convinced me that we had to deal with a shoemaker's lapstone of Vesuvian lava, the patina being nothing more than the polish of grease, dirt, with wear and tear. A small fragment was given me, which, after being sectionised, showed a typical leucitophyre of Vesuvius—probably the lava of 1631 from "La Scala" quarries.

I should not have written to you had it not been that such confirmation had been given by men of position, whom I have now obliged to admit their mistake. Probably, however, the report of the fall of this supposed aërolite has already spread, so that I fear it may be included in lists of historic meteorites.

I may say that the stone had probably fallen or been thrown from one of the roofs of the neighbouring high houses.

December 9 H. J. JOHNSTON-LAVIS

The Rotation-Period of Mars

IN the number of NATURE of November 26 (p. 81), Mr. Proctor mentions one or two points in my investigation of the rotation-period of Mars, requiring correction.

The first is, that I did not use Proctor's final result for the period, but one which he published in 1869, differing 0·02s. from the former. As I intended to determine from the whole series of Mars's drawings the correction of the rotation-period, it was perfectly indifferent what value I adopted in my calculations; the only condition was, the error should not be so great as to cause an erroneous interpretation of the Mars pictures of former years. A difference of 0·02s. in the adopted rotation-period changes the position of the markings in the drawings of Huygens and Hook but 2°·9, so that for my purpose I could adopt Proctor's value of 1869 as well as that of 1873. I chose the former, as it seemed, after a preliminary reduction of Schrœter's observations, to be nearer the truth; but my results would have been absolutely the same had I chosen the second.

The second remark of Mr. Proctor's is the following:—"Prof. Backhuyzen, like Mr. Denning some time since, has taken Kaiser's result uncorrected for the clerical errors—very seriously affecting it—which I detected in 1873." As I have used only Kaiser's *original observations*, and no *result whatever, corrected or uncorrected*, I must conclude that Mr. Proctor has not read my paper very accurately; when he does so, he will see that he is wrong. At the same time he can see that on p. 58 the time of Hook's observations is given, "March 12, 12h. 20m. and 12h. 30m.," and from the indication on p. 55, that there is an integral number of revolution-periods of 24h. 37m. 22·74s. between 1862, November 1, 9h. 55m. mean time Berlin, and 1672, January 1, 22h. 11·0m. mean time Paris, he may conclude, after a slight calculation, that I did not count the years 1700 and 1800 as leap-years. My results are, therefore, free from the errors Mr. Proctor indicated; I hope I made no other.

It seems, however, very difficult to avoid them wholly. Mr. Proctor, for instance, who occupied himself so much with the subject, writes, in the above-mentioned number of NATURE of November 26: "Kaiser counts three days too many in comparing Hook's observation with his own: one day, through a mistake in correcting for change of style and two days (apparently) from counting the years 1700 and 1800 as leap-years." That number of *three* days must be *one* day, for Kaiser indicated as the time of Hook's observations, March 13, 12h. 20m. new style, instead of March 12; by this error the number of days from 1862, November 1, till Hook was one day too small; but, by counting 1700 and 1800 as leap-years, Kaiser added two days too many, so that the total error was one day, and not three. Mr. Proctor's conclusions, based on the latter assumption, are naturally erroneous.

H. G. VAN DE SANDE BAKHUYZEN

Leyden Observatory, December 9

Ventilation

IN NATURE for December 10 (p. 132) I note the suggestion:—"In all new buildings where efficient ventilation is desired, it would be preferable to construct a shaft at one side of, or surrounding the chimney-flue, with an inlet near the ceiling of the room, and the outlet at the level of the chimney-top, so that the air escaping from the room would have its temperature kept up by contact with the chimney, thus aiding the up-draught, whilst the risk of reflux of smoke would be avoided." In building my own house some eight years ago this system was adopted in every room, the outlet over the chandelier being carried across to the side of the chimney of the same room, the two flues being carried up side by side to the chimney stack, each outlet having its own cowl. In practice this has proved a total failure, from the simple fact that the fire-flue is both longer, owing to its starting at a lower level, and that it is also hotter than the other. In the absence of any fire there is a strong upward current in both, but the instant the fire is lighted the upward current in the ceiling ventilator stops, and in a few minutes is reversed, the cold air and collected smoke from the chimney outlet coming in with such force that we have been compelled to make up every ceiling ventilator in the house except one, which, although useless when a fire is lighted, is not a nuisance. Many other experiments in automatic ventilation were tried, so that in case one system failed others might be available, and I regret to say that the only useful remnant of the experiment is the ventilation from the entrance hall, which

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apparently penetrates with good effect into every room in the house; unfortunately the flue in the entrance hall is one which has a persistent down draught, and we are unable to warm the air in the hall and passages.

There appears to be no rule without exception for automatic ventilation; in one room we have Tobin's ventilators, the opening on the outside facing direct north; these have to be closed always when the room is occupied, as the cold air, after rising a short distance, descends on the heads of the occupants. In another room are four similar shafts built in the wall, the bottom inlets facing west; these shafts are always left full open in the severest weather, the draught being usually downwards and outwards; but why this occurs I cannot form the remotest conception, unless eddies are caused by an adjoining building. There is no doubt that external conditions affect the practical ventilation of any building, and no absolute rules are applicable in every case alike. THOS. FLETCHER

Warrington

Rain at Smyrna

THE telegraph has informed us of a heavy fall of rain at Smyrna on Wednesday, November 25, but few particulars have yet reached us, except some from Capt. Stabb, Corr. Mem. Soc. of Arts. The storm began at six in the morning, and in a few hours 6 inches were registered. When the quay, or embankment, was proposed some years ago, in conjunction with the Council of Public Works I endeavoured to provide an efficient outfall sewer underneath it, but we were defeated by local intrigue. The drainage of Smyrna is worse than in the time of Strabo, by the large additions taken in from the shelving bay. The torrents pouring down from Mount Pagus (the Castle Hill) came through the Turkish quarter, causing the fall of some twenty houses, and washing bodies out of the Turkish and Jewish graveyards. On reaching the lower streets the sewers ceased to act, and cellars, stores, and warehouses were flooded, causing a loss of 50,000*l*. worth of opium, cotton, valonia, and other merchandise, and much damage to private houses. The River Meles overflowed its shallow bed, and reached the Point Railway Station, destroying some market gardens. In the suburb of Bournabat two houses were brought down, and the Cassuba railway embankment was damaged. The storm seems not to have reached beyond Manisa (Magnesia ad Sipylum), over Mount Sipylus, on one side, and Turbalu, beyond the Smyrna plain, on the other. No such inundation has taken place within memory. HYDE CLARKE

The Sea-Mills at Argostoli

WITH reference to the inquiry of your correspondent, Surgeon Lloyd Thomas, in your issue of the 10th instant (p. 129), there is a short paper on this subject by Capt. H. P. Shilston, entitled "On Curious Natural Phenomena in Cephalonia," to be found in the *Transactions* of the Liverpool Geological Association, vol. i. (Liverpool: Henry Young, 1881). The writer describes the inflow of water to the land through crevices in the limestone, as observed by him, and gives an explanation by Prof. Ansted, M.A., F.R.S., who considers that the phenomenon arises from the large amount of evaporation, within range of the district, by which the level of the subterranean stores of water is kept constantly below the level of the sea, notwithstanding the joint supply of rain- and sea-water. O. W. J.

Liverpool, December 12

IN answer to the question of Mr. J. Lloyd Thomas in the last number of your valuable paper (p. 129) respecting the sea-mills of Argostoli, we beg to inform you that we have published, "Die Insel Kephalonia und die Meermühlen von Argostoli, Versuch einer Lösung dieses geophysikalischen Räthsels," von Prof. K. W. M. Wiebel, mit 1 Karte, 3 Skizzen und 5 Holzschnitten, 1873.

Hamburg, December 12 L. FRIEDERICHSEN AND CO.

Friction and Molecular Structure

I SHALL feel obliged if you will kindly allow me to ask any reader of NATURE whether moderate friction can so change the molecular structure of glass as to account for the following fact:—Last night, about twenty minutes after a paraffine lamp had been lighted and had been burning steadily, its glass chimney suddenly burst into small fragments at the exact place at which, about an hour before, I had rubbed it with a piece of brown paper in order to remove soot from the interior. The chimney was thoroughly annealed, having been in constant use for more than three years. The flame was not high; the night was not frosty; the glass was uniformly thin at the place of fracture, which was six inches above the top of the flame, and two inches below the top of the chimney. The part which had not been rubbed is quite uninjured: not even a crack extending into it, while the rubbed part is shattered. EDWARD GEOGHEGAN

Bardsea, December 1

The Resting Position of Oysters—A Correction

MR. J. T. CUNNINGHAM in his letter of November 28 (p. 129), after showing that *Pecten opercularis* must rest on its right valve, goes on to say:—Of *Pecten maximus* I cannot speak with certainty, and therefore leave to Mr. Arthur Hunt the responsibility of stating that there is a difference in respect of position in the two species." So far from my having hinted that any species of Pecten rests on other than the right valve, my letter, to which Mr. Cunningham refers, concludes with the plain statement, "in each case the mollusk rests on the same valve." The point to be noticed is that in *Pecten maximus* the right valve is most convex, and in *Pecten opercularis* the left valve.

A. R. HUNT

Radiolaria

I HAVE recently had the pleasure of finding, in the London Clay, a number of well-preserved specimens representative of several species of Radiolaria, most of which, I have good reason for thinking, differ from any known fossil or recent forms. It was my intention to submit them to the Geological Society during the present month, but circumstances prevent this being done. The delay may lead to an extension of the list, especially if I am fortunate enough to meet with a microscopist kind enough to assist in the examination of material yet untouched. W. H. SHRUBSOLE

Sheerness-on-Sea, December 14

THE CONTINUITY OF THE GERM-PLASMA CONSIDERED AS THE BASIS OF A THEORY OF HEREDITY[1]

THE thoughts developed in this most interesting and important essay were first expressed in a lecture delivered to students of the University of Jena last winter. They were reduced to writing in the spring, and completed for publication in June. The author received Oscar Hertwig's essay on the "Theory of Inheritance," and Kölliker's "On the importance of the Cell-nuclei for the Processes of Heredity," after his manuscript was complete. In the matter of the extreme importance of the nucleus he agrees with both these authors.

As was stated in reviewing here two years ago Prof. Weismann's memoir "On the Origin of the Sexual Cells of the Hydromedusæ," all his memoirs abound in original views and suggestions, which render them of peculiar and widely-spread interest. The present is no exception to the rule. It is intended in this article to give a kind of abstract of the memoir, composed largely of a series of translated passages: for the fuller development of details, the history of the development of ideas on the subject, and controversial matters, readers are referred to the original, which is an octavo of 122 pages.

"How is it," asks the author in commencement, "that in the case of all higher animals and plants, a single cell is able to separate itself from amongst the millions of most various kinds of which an organism is composed, and by division and complicated differentiation to reconstruct a new individual with marvellous likeness, unchanged in many cases even throughout whole geological periods?" The question is a hard one indeed, and the various attempts which have been made to solve it,

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[1] "Die Continuität des Keimplasma's als Grundlage einer Theorie der Vererbung." Von Dr. August Weismann, Professor in Freiburg i. B. (Jena: Verlag von Gustav Fischer, 1885.)

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